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Peer review report

Peer review report 1 on "Regional climate change projections of streamflow characteristics in the Northeast and Midwest U.S."

1. Original Submission

1.1. Recommendation

Major Revision

2. Comments to Author:

Manuscript Review for Journal of Hydrology: Regional Studies, August 2015
Manuscript Number: EJRH-D-15-00146
Title: Regional climate change projections of streamflow characteristics in the Northeast and Midwest U.S.
Authors: Demaria, Palmer, Roundy
GENERAL COMMENTS:

This paper addresses a topic of interest to the readers of Journal of Hydrology: Regional Studies. The manuscript is clearly and concisely written. While the statistical methods are sound, a few questions about specific methodological choices remain. In addition, the statistical results, and the discussion section, are not presented in context of the literature or of the physical mechanisms behind the results (as discussed in the following two paragraphs). I am recommending acceptance of this manuscript on condition of major revision, even though I do not think that the revisions require a great deal more statistical analysis.

The authors should frame their statistical results more in context of the literature. They provide a nice literature review, but then do not sufficiently refer back to the literature in either the results or discussion sections. For example, p. 22, lines 488-492, they find that the models predict a decrease in extreme flooding events in the Northeastern US by mid-century. This is a very interesting result, which is opposite to what has been observed with this region having the highest increase in the US. How do their results relate to the results of other studies? Also, the authors should relate their results to the results of: Thibeault and Seth, Changing climate extremes in the Northeast United States: observations and projections from CMIP5, Climatic Change (2014) 127:273-287.

The authors should frame their statistical results more in the context of physical explanation. For example, over the northeastern US, they find an increasing trend in large precipitation events, an increasing trend in baseflow, but a decreasing trend in the largest streamflow events (p. 25 lines 557-562; Figures 3, 5, 6). This does not make obvious intuitive sense. The most likely explanation is related to the seasonality of the trends in relation to the seasonality of extreme event magnitudes, but neither this nor any other explanation is offered. For example, the authors should consider the results of: Frei et al., 2015, The Seasonal Nature of Extreme Hydrological Events in the Northeastern United States, Journal of Hydrometeorology, in press, available for early on line release at http://journals.ametsoc.org/doi/abs/10.1175/JHM-D-14-0237.1); Furthermore, with the advantage of having the model results, these changes should be explained in a physical sense more completely.

SPECIFIC COMMENTS:

DOI of the original article:http://dx.doi.org/10.1016/j.ejrh.2015.11.007.

2214-5818/$ – see front matter
http://dx.doi.org/10.1016/j.ejrh.2016.01.030

1. p. 12, starting on line 254. Please be more explicit about justifying the choices of the metrics. Justify the use of 3-day peak flows, 7-day low flows, and mean base flows, 5 day cumulative precipitation. Are the results sensitive to these specific choices?

Section 2.3. Justify the use of monthly GCM results. There are CMIP5 GCMs with daily results available. Why not use those? There are both advantages and disadvantages, but the use of daily model results would allow one to look at different sequences of wet/dry days. This may affect some of the results. The GCMs produce a range of wet-day/dry-day sequences, which may affect the production of large streamflow events in the VIC model. (Also see specific comment #5.)

3. p. 14 sec 3.1, and p. 26, line 576-578. The identification of "best" GCMs, using REA, includes in addition to a measure of the similarity to historical variations, a measure of the similarity to the GCM-ensemble-mean for the future scenarios. Why is the similarity to the ensemble mean of future scenarios a criterion for "best"? This is related to the discussion on p. 26, line 576-578. By choosing simulations that are more similar to each other the authors state that they are reducing uncertainty. This seems like circular reasoning.

4. p. 16, lines 341-346. What is meant by mismatch? Please demonstrate more clearly how well the magnitudes and frequency of peaks/lows are simulated by the model.

5. p. 18, lines 391-403. How much are simulated soil moisture changes affected by excluding changes in # wet days? The authors state that the frequency of events is important, but they are not considering whether GCMs predict a change in the frequency of events (not just extremes). This is related to specific comment #2.

6. p. 19, lines 410-417. It seems that the authors use the annual max 5 day precipitation event as a proxy for antecedent conditions. It is not explained, nor is it intuitively obvious, how this is a logical proxy for antecedent conditions. This would identify the extreme events, not antecedent conditions. Please either clarify what you have done, justify the use of 5 day max precipitation as a proxy for antecedent conditions, or use a different proxy.

7. p. 20, lines 433-447. GCM-driven and observation-driven hydrological changes are different! What does this tell you about the GCM results, and how does this affect the conclusions? The authors should address this question.

8. p. 24, lines 523-528. Please provide more adequate explanation (not just description, but physical explanation) of difference in the results between scenarios. This is a big difference, but not explained. Having the model output should enable some explanation.

MINOR COMMENTS:

There are two sections labeled "section 2.3"

2. p. 10. Equations for REA - the equations, terms in the equations, and meanings of the equations require more explanation

3. p. 12 line 269. Should "correlated" be "auto-correlated"? Does this procedure adequately account for the seasonal cycle?

4. p. 14, lines 298-300. The authors state that "Since warmer temperatures and little change in precipitation are likely to occur during the warm months." But the table indicates statistically significant changes in precipitation during summer. Please explain.

5. p. 14, lines 356-359. Please state the null hypothesis more clearly

6. p. 17, lines 366-368. Are variables other than discharge normalized by basin area to be compared to precip, ET, etc..?

7. table 2. Since most numbers are significant, and therefore in bold, perhaps better to keep significant values in normal font, and identify insignificant values it italics or with parentheses.

Table 2 Would including a discussion of simulated changes in P-E be useful?

9. fig 5 - both middle and bottom row are labelled rcp 4.5

10. p. 21, lines 466-468. The authors state that "Increasing trends in 4% of the basins are projected for the GCM-ensemble for 7-day low flows by mid-century with most of the basins located in the eastern half of the domain (Figure 5e)." On the figure it looks like more than 4% of the stations. Please confirm that this is correct.

11. p. 21, lines 501-503. Please clarify - is the threshold computed based on the historical period, and then used for all periods?

12. Please explain the interactive map link at end. It did not seem to work for me.

Anonymous
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